When the heating was not excessive the bubbles did not become motionless, but their vibrations had a tendency to one side of the cavity, as in fig. 7, D and E.

The larger bubbles in other cavities were likewise attracted. The vibrating bubbles were generally seen at or near the top of the cavities. Cooled with ether-spray the bubbles ceased vibrating. The microscope-stage was inclined at an angle of 45° throughout the experiments. About fifty different cavities, none of them exceeding the dimensions already given, all behaved in the same way. There were several moving bubbles in cavities a little larger than those already noted; for instance one measured $\frac{1}{4000} \times \frac{1}{5000}$ of an inch. The motion never extended across the cavity, but was confined to a sort of shuffling up and down, which shifted the bubble from one side to the other (see fig. 7, D and E). On presenting a warm wire to the cavity the bubble was instantly attracted, and it remained clinging for some time to the side of the cavity.

For an explanation of the cause of vibration I must refer to the fact that I have proved, that gas-bubbles in water as well as in carbonic acid may be attracted by a source of heat giving an extremely slight rise of temperature. It is impossible to imagine a body which is not gaining or losing, or, at the same time, both gaining and losing heat; it is therefore impossible to imagine it entirely throughout at a uniform temperature. It is evident, then, that an easily movable particle which can be set in motion by exceedingly slight rises of temperature will make the transference of heat from one point to another plainly visible. I have shown that the minute bubbles in fluid-cavities are such particles; and I believe that the vibratory motions which I have described afford an ocular demonstration of the continual passage of heat through solid substances.

April 19, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Putrescent Organic Matter in Potable Water." By Gustav Bischof. Communicated by E. Frankland, F.R.S. Received March 17, 1877.

It is fortunate that smell and taste are generally extremely sensitive indicators of putrefaction in articles of food. This does not, however, apply to drinking-water, which may be largely polluted by putrescent organic impurities without causing any suspicion to our

senses. And yet the question of the wholesomeness of water hinges mainly upon the presence or absence of such putrescent matters, as they themselves are the cause of derangements of the human system. Most serious, however, are the consequences when those low forms of organic life, which in all probability form the specific poison of cholera, typhoid fever, and other diseases, gain admission to drinking-water polluted by putrescent matter.

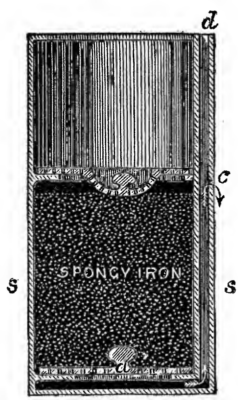
A number of observations point to the conclusion that these organisms, or their germs, are not infectious as long as surrounded by *fresh* organic matter; but as soon as fermentation sets in, they show their poisonous virulence. Thus it has been observed that the discharges of cholera and typhoid patients are not infectious as long as they are fresh, but by putrescence their poisonous character is developed.

Chemical analysis is incapable of discriminating between living or dead, fresh or putrescent organic matters. The microscope reveals their nature more fully; but it is nevertheless frequently a matter of great difficulty to decide as to the existence or non-existence of *Bacteria* of putrefaction, or their germs, in water. It thus appeared to me that this information might, in some cases at least, be gained with greater certainty by an indirect method.

If we want to determine whether a gas be carbonic anhydride, we pass it through potash bulbs, and see whether these increase in weight. Similarly the presence or absence of putrefactive agencies in water may be determined by their action upon organic matter. The test I selected is fresh meat, as the slightest putrescent changes in it can most readily be detected by its smell.

The experiments, which were originally made with a view of determining the improvement of water by certain filtering media, were, with the exception of experiment VIII., carried out in the following manner:—

On to the perforated bottom (*a*) of a stoneware vessel (*s s*) I place



some fresh meat. The vessel is then filled to about two thirds with the

materials to be experimented upon, and lastly with water. Into opening *c* a tin tube is fixed, which is first bent upwards and then downwards in the shape of an inverted U, to prevent any *Bacteria* or their germs from passing through this outlet-tube into the bottom of the vessel. The air-pipe *d*, down to *c*, is filled with firmly compressed cotton-wool, and a glass tube, sealed at its bottom, is passed down through the material experimented upon, to allow of the temperature being measured in close proximity to the meat. The vessels thus prepared are immersed in a boiler filled with cold water, which is gradually heated and kept boiling for several hours. The object of this is to destroy any germs adhering to the meat. The temperature at the bottom of the sealed glass tube was, during the boiling, in each of the following experiments 93°–95° C.

After cooling, the Chelsea Company's water was constantly passed through the vessels in the direction indicated by arrows, at as nearly as possible a uniform speed.

It is thus evident that any *Bacteria* of putrefaction, or their germs, in the water would, after a time, render the meat putrid, or, if it remains fresh, they must have been absent, or at least inactive, when the water reached the meat.

I now proceed to describe the experiments.

Experiment I.—One of the vessels was filled with spongy (metallic) iron, and treated as before described; after a fortnight the meat was perfectly fresh.

Experiment II.—A vessel filled with animal charcoal; after a fortnight the meat showed strong evidence of incipient putrefaction. As experiments I. and II. were conducted side by side, this result proves that the preservation of the meat in experiment I. was not due to any external cause, such as the low temperature then prevailing.

Experiment III.—Water continuously passed through a vessel filled with spongy iron for four weeks; even then the meat was perfectly fresh and hard.

Experiment IV. was a repetition of II., the filtration of water through animal charcoal being continued for four weeks. The meat was soft and quite putrid. In the course of this experiment the exit-tube was several times choked by mucous matter.

Experiment V.—In experiments I. and III. with spongy iron, this material was employed without separating any of the fine dust. In order to ascertain whether *Bacteria* were merely mechanically retained, a vessel was charged with spongy iron, from which all the finer particles had been separated by a sieve with thirty holes on the linear inch. The filtering medium in this case was therefore of a porous nature. After four weeks' filtration the meat was perfectly fresh.

Experiment VI.—In the previous experiments with spongy iron the meat was in contact with water, from which the iron in solution had not been separated. With a view of ascertaining whether the iron in solution

was the preserving agent, a stoneware vessel was charged underneath the spongy iron with pyrolusite and sand, so as to abstract the iron from the water before it came in contact with the meat. After four weeks' filtration the latter was found perfectly fresh.

Experiment VII.—By a separate experiment I ascertained that the oxygen is completely abstracted from water during its passage through spongy iron. In order to determine whether the absence of oxygen be the cause of the preservation of the meat, and whether the *Bacteria* or their germs be killed or can be revived when supplied with oxygen, an evaporating-basin was inverted over the meat. This must have retained a quantity of air in its cavity, the air being gradually dissolved by the water in close proximity to the meat. After four weeks' filtration the meat was perfectly fresh; I succeeded in collecting a small bubble of the gas, still in the cavity of the evaporating-basin. This was quite free from oxygen.

It is therefore doubtful whether oxygen was supplied to the water sufficiently long to justify any conclusions from this experiment. However, the result of experiment VIII. rendered a repetition unnecessary.

Experiment VIII.—Fresh meat was placed at the bottom of a glass vessel and left standing, covered with about four inches of spongy iron and water. The vessel in this instance was *not* boiled. After three weeks the meat was very bad, demonstrating that the action of the *Bacteria* of putrefaction adhering to the meat was not prevented by the spongy iron above; and if, during the previous experiments with spongy iron, agencies capable of causing putrefaction had at any time come in contact with the meat (in other words, if the *Bacteria* had not been killed in their passage through spongy iron), the meat must, as in this last experiment, have shown marks of their action. It therefore appears that *Bacteria* are permanently rendered harmless when passing in water through spongy iron. This conclusion is further corroborated by the observation that even effluent sewage-water, after passing through the spongy material, has remained perfectly bright for now five years when exposed to light in a half-filled stoppered bottle.

I believe that the action of spongy iron on organic matter largely consists in a reduction of ferric hydrate by organic impurities in water. We know that even such organic matter as straw or branches is capable of reducing ferric to ferrous hydrate. We know that even such indestructible organic matter as linen and cotton fibres is gradually destroyed by rust-stains. This action is slow when experimenting upon ordinary ferric hydrate; but it may, *in statu nascendi*, be very energetic—the more so, if we consider the nature of the organic matter in water. Ferric hydrate is always formed in the upper part of a layer of spongy iron when water is passed through that material. The ferrous hydrate resulting from the reduction by organic matter may be re-oxidized by oxygen dissolved in the water, and thus the two reactions

repeat themselves. This would explain why the action of spongy iron continues so long.

It is, however, quite certain that there is also a reducing action taking place when ordinary water is passed through spongy iron. This is clearly indicated by the reduction of nitrates.

Our knowledge of those low organisms which are believed to be the cause of certain epidemics is as yet too limited to allow of direct experiments upon them. It is not improbable that, like the *Bacteria* of putrefaction, they are rendered harmless when water containing them passes through spongy iron; but until we possess the means of isolating these organisms, this question can only be definitively settled by practical experience. Should this not be satisfactory, should those specific contagia not be destroyed when passing in water through spongy iron, then the separation of *Bacteria* by spongy iron may afford means of isolating those germs of disease; should it be favourable, then we shall have found in spongy iron the material to prevent the spreading of epidemics by potable water.

II. "On a Cause for the Appearance of Bright Lines in the Spectra of Irresolvable Star Clusters." By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope. Received March 20, 1877.

Before the announcement of Mr. Huggins's discovery of the presence of bright lines in the spectra of nebulæ, it was generally, if not universally, accepted as a fact that nebulæ were merely stellar clusters irresolvable on account of their great distances from us. This view had become impressed on the minds of many of our greatest observing astronomers in the progress of their work, and is one therefore which should not lightly be abandoned.

It appears to me that Mr. Huggins's observations, instead of being inconsistent with the view formerly held by astronomers, are rather confirmatory of the correctness of that view.

The sun is known to be surrounded by a gaseous envelope of very considerable extent. Similar envelopes must surround the stars generally. Conceive a close stellar cluster. Each star, if isolated, would be surrounded by its own gaseous envelope. These gaseous envelopes might, in the case of a cluster, form over the whole, or a part of the cluster, a continuous mass of gas. So long as such a cluster was within a certain distance from us, the light from the stellar masses would predominate over that of the gaseous envelopes. The spectrum would therefore be an ordinary stellar spectrum. Suppose such a cluster to be removed further and further from us. The light from each star would be diminished in the proportion of the inverse square of the distance; but such would not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